NBER WORKING PAPER SERIES

DO CONSUMERS EXPLOIT PRECOMMITMENT OPPORTUNITIES? EVIDENCE FROM NATURAL EXPERIMENTS INVOLVING LIQUOR CONSUMPTION

B. Douglas Bernheim
Jonathan Meer
Neva K. Novarro

Working Paper 17762
http://www.nber.org/papers/w17762

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
January 2012

We are grateful to Jennifer Doleac, Mark Hoekstra, Michael Lovenheim, Harvey Rosen, Jeremy West, and William Gui Woolston for valuable comments. Numerous state alcohol board agents, law librarians, and legislative assistants contributed clarification on data issues; Michael Lovenheim and Mark Stehr provided supplemental data. Jillian Carr, Mario Garza, Mustafa Karakaplan, Analisa Packham, Xander Slaski, and Zhihao Zhang provided able research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2012 by B. Douglas Bernheim, Jonathan Meer, and Neva K. Novarro. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Do Consumers Exploit Precommitment Opportunities? Evidence from Natural Experiments Involving Liquor Consumption

B. Douglas Bernheim, Jonathan Meer, and Neva K. Novarro

NBER Working Paper No. 17762
January 2012, Revised November 2013
JEL No. D03,D12,H31,I10

ABSTRACT

This paper provides evidence concerning the extent to which consumers of liquor employ precommitment devices. One widely recommended precommitment strategy is to regulate alcohol consumption by deliberately manipulating availability. We assess the prevalence of the "availability strategy" by assessing the effects of policies that would influence its effectiveness – specifically, changes in allowable Sunday sales hours. We find that consumers increase their liquor consumption in response to extended Sunday on-premise sales hours, but not in response to extended off-premise sales hours. The latter finding is inconsistent with widespread use of the availability strategy.

B. Douglas Bernheim
Department of Economics
Stanford University
Stanford, CA 94305-6072
and NBER
bernheim@stanford.edu

Jonathan Meer
Department of Economics
TAMU 4228
College Station, TX 77843
and NBER
jmeer@econmail.tamu.edu

Neva K. Novarro
Department of Economics
425 N College Ave.
Claremont, CA 91711
nnovarro@gmail.com

1. Introduction

Over the last twenty years, the concept of time inconsistency has emerged as a central theme in behavioral economics, one that has begun to influence thinking concerning public policy.[1] As is well-known, any consumer sufficiently self-aware to notice her time-inconsistent tendencies will manifest a demand for precommitment technologies. At a minimum, consumers should acquire such self-awareness with respect to frequently repeated activities for which they consistently fail to follow through on prior intentions. Yet oddly, there is surprisingly little evidence that people actually value and exploit precommitment opportunities.[2] A collection of relatively recent papers has begun to fill that gap.[3] Still, nagging doubts persist, partly because much of the evidence is equivocal, and partly because its scope is limited.[4] Skeptics continue to wonder why, if time inconsistency is so prevalent, the free market provides so few precommitment devices, and unambiguous examples in the field are so difficult to find.[5] Indeed, some suggest that the fewness of the obvious exceptions proves the rule.

The consumption of addictive substances is often offered as a leading example of time inconsistency. Becker and Murphy (1988) have shown that a consumer with time-consistent preferences could exhibit many of the choice patterns associated with such substances. However, unlike certain behavioral theories of addiction that envision various forms of time inconsistency, such as quasi-hyperbolic discounting (Gruber and Koszegi, 2001), cue-triggered visceral modes (Bernheim and Rangel, 2004), and temptation preferences (Gul and Pesendorfer, 2007), the Becker-Murphy model does not imply a demand for precommitment. Despite the centrality of this implication, evidence that users of addictive substances actually value precommitment op-

---

[1] Examples of policy applications include Laibson (1997, 1998), Laibson, Repetto, and Tobacman (1998), Krussell, Kuruscu, and Smith (2000), Diamond and Koszegi (2003), Thaler and Benartzi (2004), Bernheim and Rangel (2004), Amador, Werning, and Angeletos (2006), Fang and Silverman (2009), Banerjee and Mullainathan (2010), Bernheim, Ray, and Yeltekin (2013), and many others.

[2] Most of the pertinent literature echoes this evaluation. For example, Gine, Karlan, and Zinman (2010) write that "there is little field evidence on the demand for or effectiveness of such commitment devices." For recent surveys, see Bryan et al. (2010) and DellaVigna (2009).

[3] See in particular Ariely and Wertenbroch (2002) on homework assignments, Ashraf, Karlan, and Yin (2006) on commitment savings devices in the Phillipines, Beshears, Choi, Laibson, and Madrian (2011) on commitment savings devices in the U.S., Houser et al. (2010) for a laboratory experiment in which subjects gain relevant experience, and Kauer, Kramer, and Mullainathan (2011) on incentive schemes.

[4] For example, in Ariely and Wertenbroch's experiment, students may have been motivated by a misguided desire to signal diligence. Likewise, much of the evidence on the demand for commitment savings products in developing countries is potentially attributable to other-control (i.e., family and friends) than to self-control; see, e.g., Dupas and Robinson (2011).

[5] Many common financial products, such as mortgages and retirement accounts, entail precommitments. However, those products offer other advantages, and it is not clear whether their inflexibility increases or reduces demand.

portunities is almost entirely limited to anecdotes. An important recent exception is Gine, Karlan, and Zinman (2010), who study the use of a commitment device for smoking cessation, but the general lessons to be drawn from their experiment are unclear.[6] Without more systematic and extensive evidence, it is impossible to know whether the anecdotes pertain to commonplace or exceptional modes of behavior.

Some evidence from clinical practice actually casts a degree of doubt on the hypothesis that addicts value precommitment opportunities. For example, alcoholics can commit to sobriety by taking disulfiram, a drug that produces an unpleasant reaction to alcohol. However, only *supervised* disulfiram administration is generally recognized as effective; compliance is poor among patients who are given the drug to take on their own (see, e.g., Hughes and Cook, 1997, and Anton, 2001). Whether this pattern reflects the absence of a demand for precommitment or a deficiency of this particular precommitment device remains unclear.[7]

A simple method for exercising self-control is to limit the availability of a problematic good by not maintaining an easily accessed supply. For example, dieters are counseled against keeping fattening foods at home, while smokers and alcoholics who wish to quit or at least control their habits are often given similar advice.[8] We will refer to this self-control technique as the *availability strategy*. Notice that it involves a partial precommitment: while it does not prevent an individual from obtaining the good in question, it restricts future opportunities in a way that makes the targeted behavior less convenient. When enumerating popular precommitment devices, economists regularly cite the use of the availability strategy to control alcohol consumption. According to Schelling (1988), the strategy is to "[r]emove the mischievous resources: don't keep liquor, or sleeping pills, in the house." Likewise, Bryan, Karlan, and Nelson (2010) describe a handful of techniques to illustrate "ad hoc commitment devices," which includes "not keeping alcohol in the house."

---

[6] Subjects were offered the opportunity to post a bond, the principle of which was forfeited to a charity if they failed a urine test after six months. It is not clear whether the relatively low take-up rate (11 percent) indicates a rather small demand for precommitment devices in general or for this device in particular. Also, either generosity or a desire to signal generosity toward the charity may have artificially inflated the take-up rate (particularly inasmuch as two-thirds who participated failed to quit smoking).

[7] Disulfiram does not directly suppress alcohol cravings. Thus, when an alcoholic takes disulfiram, he runs the risk that he will give in to cravings and experience an extremely unpleasant reaction.

[8] Notably, the National Institute on Alcohol Abuse and Alcoholism (a division of the NIH) maintains a website called "Rethinking Drinking," on which it offers "Tips to Try" for those who have not decided to give up alcohol entirely but want to cut down. The NIAAA recommends that "[i]f drinking at home is a problem, keep little or no alcohol there." See http://rethinkingdrinking.niaaa.nih.gov/Strategies/TipsToTry.asp (accessed January 9, 2012). This recommendation is echoed in many other self-help resources.

In this paper, we look for evidence that consumers actually employ the availability strategy to control their liquor consumption. We exploit a collection of state-level policy changes that altered the hours during which consumers can purchase liquor on Sundays (so-called "blue laws"). Someone who attempts to control liquor consumption by keeping no liquor at home will have greater success (at least on Sundays) in a regime with strict blue laws. Thus, under the hypothesis that the availability strategy is prevalent, a relaxation of blue laws should lead to a noticeable increase in liquor consumption. Of course, the same is true for a time-consistent consumer: assuming (plausibly) that liquor is complementary to restaurant meals and socializing at bars, then those who like to dine out and/or socialize on Sundays will consume less liquor in a regime with stringent blue laws. Thus, the mere fact that blue laws reduce liquor consumption – a proposition that already has some support in the literature (see Section 2) – cannot discriminate between the hypotheses of interest. A more discerning test is needed.

A critical feature of our analysis is that it distinguishes between restrictions on *off-premise* liquor sales (through liquor and package stores) and *on-premise* sales (in restaurants and bars).[9] For a practitioner of the availability strategy, *both* types of restrictions make the acquisition of alcohol significantly more difficult when none is kept at home; hence both should reduce liquor consumption. In contrast, time-consistent consumers should respond to off-premise restrictions by carrying "inventories."[10] Liquor consumption among such consumers will be affected by Sunday off-premise sales restrictions only if consumers are forgetful, or if the costs of carrying inventories are large. Thus, a model of time-consistent consumption can easily account for the absence of a relationship between liquor consumption and *off-premise* sales restrictions (indeed, we take this to be the most natural implication of such models).

We investigate these issues by examining the effects on annual state-wide liquor purchases of changes in state laws defining allowable Sunday hours for liquor sales. We focus on liquor rather than wine or beer for two reasons: first, sales restrictions are generally simpler and easier to code for liquor than for beer and wine;[11] second, there is evidence that the self-control prob-

[9] The precise definitions of off-premise and on-premise sales are set by state licensing laws.

[10] Rational time consistency has a different implication for on-premise sales restrictions because alcohol in bars and restaurants is not storable (consumers cannot hold inventories), and consumes are not completely indifferent with respect to going out on different days of the week.

[11] Laws pertaining to beer and wine sales tended to be more complicated, with many states carving out exemptions that are difficult to code. For example, particularly during the earlier part of our sample, some of these laws distinguished between different types of beer and wine based on alcohol content. In comparison, laws governing the sale of distilled spirits were generally more straightforward.

lems commonly associated with alcohol consumption are most prevalent for liquor (see, for example, Kerr *et al*., 2002, or Roizen *et al*. 1999). We examine 32 changes in allowable off-premise hours and 46 changes in allowable on-premise hours occurring between 1970 and 2007; in 22 of these cases, states changed both limits during the same year. We estimate panel regressions with state fixed effects (to control for unobserved differences in attitudes toward alcohol that may affect both regulations and consumption) and year effects (to control for general trends in liquor consumption over time). Thus, we identify the effects of restrictions on allowable Sunday sales hours by, in effect, comparing the changes over time in liquor purchases for states that modified their regulations to the changes for states that did not. We address some threats to our identification strategy below.

Our central finding is that liquor consumption increases along with allowable on-premise Sunday sales hours, but there is no evidence that it is affected by off-premise Sunday sales hours. These findings are robust with respect to a wide variety of specifications, including ones that control for pre-existing trends and concurrent changes in related restrictions. Thus, to our considerable surprise, we find no indication that the availability strategy plays a meaningful role in aggregate liquor consumption. Instead, the observed pattern coincides with our prediction for time-consistent consumers who have rational expectations and low costs of carrying inventories. Naturally, the possibility remains that liquor purchasers are time-inconsistent, but that they favor some other technique for exercising self-control; for instance, Bernheim, Ray, and Yeltekin (2013) demonstrate that a time-inconsistent decision maker may avoid external commitments because they undermine internal self-control strategies.

In addition to shedding light on the demand for precommitment devices, our analysis also has direct implications for tax policy. Over the last few decades, many states have repealed or relaxed blue laws in an attempt to increase tax revenues. The efficacy of these measures is controversial. For example, a recent article in the *New York Times* cited projections that permitting Sunday sales would increase tax revenues in Georgia, but also referenced unnamed analysts who "are less certain that [this step] will bring in much new money because drinkers are likely simply to shift the days they buy alcohol" (Severson, 2011). Our analysis informs that debate by showing that relaxing restrictions for on-premise sales is likely to increase revenue, but doing so for off-premise sales is not.

2. Relation to the Existing Literature on the Effects of Blue Laws

There is a small but growing literature concerning the effects of blue laws on the consumption of alcohol and alcohol-related behaviors. However, that literature has not explored the implications of those effects regarding time-inconsistency and the demand for precommitment technologies, or estimated the effects in a way that permits one to draw those implications.

Several studies have estimated the impact of blue laws on alcohol consumption. Ornstein and Hanssens (1985) evaluate the effects of several alcohol control measures using a state-level cross-section. They include a dummy variable indicating whether a state allows off-premise Sunday sales and find a positive and statistically significant relationship for beer but an implausible negative (and statistically significant) relationship for liquor. Stehr (2007) uses panel data to estimate the effects of alcohol taxes and Sunday restrictions on off-premise alcohol sales;[12] he finds that liquor consumption increases by 3 to 7 percent when a Sunday off-premise ban is repealed. Carpenter and Eisenberg (2010) provide cross-sectional evidence for Canadian provinces, and study changes in the drinking habits of Ontario residents after that province repealed its Sunday ban on off-premise sales in 1997. Based on survey data, they find an increase in drinking on Sundays coupled with a decline on Saturdays, and "no evidence that these policies affected overall population drinking rates."

Two considerations prevent one from drawing implications concerning the demand for precommitment devices from the aforementioned studies. First, cross-sectional evidence on the effects of blue laws (e.g., Ornstein and Hanssen, 1985, and much of Carpenter and Eisenberg, 2010) is suspect due to the likelihood that unobserved differences in attitudes toward alcohol affect both regulations and consumption. Second, none of these studies explicitly controls for restrictions pertaining to on-premise alcohol sales. In our sample, the correlation between allowable on-premise and off-premise Sunday sales hours is 0.57, and states changed both limits in the same year 22 times, which represents 69% of the changes in allowable off-premise hours, and 48% of the changes in allowable on-premise hours. Thus, the effects measured by the aforementioned studies (whether in cross-sections or panel data) likely reflect the blended impact of off-premise and on-premise restrictions. While this consideration is not especially problematic for their purposes, it is fatal for ours (because we are concerned with the differential impact of off-

[12] Stehr's paper does not explicitly mention whether the changes he examines pertain to off-premise or on-premise sales. He clarified this issue for us through a personal communication.

premise and on-premise restrictions). The one exception is Carpenter and Eisenberg's analysis of Ontario's 1997 liberalization; as far as we can determine, Ontario did not alter on-premise restrictions on alcohol sales at the same time. However, that evidence is limited to a single policy change; consequently, all statements concerning statistical significance presuppose the absence of any common unobserved shocks to alcohol consumption across Ontario residents.

We also depart from the aforementioned literature by studying the effects of allowable hours rather than outright bans. Information on bans is far more readily available than data on allowable hours. We undertook the time-consuming process of collecting the latter information because it permits us to employ richer characterizations of each state's policies, and because it enables us to study the effects of a much larger set of legislative events.

In principle, one can also draw inferences concerning the effects of blue laws on alcohol consumption indirectly from studies that examine alcohol-related activity – specifically, traffic accidents and crime. McMillan and Lapham (2006) found that repealing the ban on off-premise Sunday alcohol sales in New Mexico led to a 29% increase in alcohol-related traffic accidents and a 42% increase in alcohol-related traffic fatalities. Similarly, Ligon and Thyer (1993) found that a Sunday sales ban on alcohol reduced the incidence of DUIs. Smith (1990) reports on a handful of studies concerning alcohol policy changes in Australia; generally, he finds a positive relationship between increased Sunday alcohol sales and traffic accidents. Olsson and Wikstrom (1982) note that alcohol sales fell by 8% during an experimental period in 1981 when Swedish liquor stores were closed on Saturdays, with accompanying declines in domestic disturbances and arrests for drunkenness; however, they are careful to point out that causality is unclear because the experiment was nationwide. Other recent studies have found much more limited effects. Stehr (2010) examines changes in Sunday off-premise sales bans in a number of states and finds an increase in traffic fatalities only for New Mexico. Using similar data, Lovenheim and Steefel (2010), conclude that "blue laws have little effect on fatal accidents." Finally, a recent paper by Heaton (2012) showed that crime increased somewhat along with the phased introduction of Sunday package liquor sales in Virginia. Like the studies of alcohol consumption mentioned above, these analyses shed limited light on the questions motivating our investigation because they make no explicit attempt to differentiate between the effects of on-premise and off-premise sales, and/or concern single policy changes. One must also exercise caution in drawing conclusions about alcohol consumption from alcohol-related activity; for example, the absence

of a statistically significant effect on traffic accidents or crime would not necessarily imply the absence of a statistically significant effect on consumption.

The current paper is also related to recent work by Hinnosaar (2012), who estimates the effect of a ban on Sunday beer sales indirectly, rather than from studying actual policy changes. Using scanner data on beer purchases, she derives an estimate of the fraction of consumers who have time-inconsistent preferences, and then uses that model to simulate the effects of a Sunday ban under the assumption that those consumers would employ the availability strategy. Her work is complementary to ours, in that we examine actual policy changes and determine whether behavioral responses are consistent with precommitment behavior.

3. Theoretical Considerations

A simple theoretical model helps to illuminate the circumstances in which people may attempt to exercise self-control over off-premises alcohol consumption by limiting readily available supplies. For simplicity, the model has only three time periods, $t = 0,1,2$. Decisions unfold as follows:

At time 0, the consumer has a convenient opportunity to purchase one unit of alcohol. By "convenient," we mean that he shops at time 0 for other reasons, so that the incremental cost of buying alcohol is limited to its price. For simplicity, no consumption occurs at time 0, but any alcohol purchases are costlessly storable.

At time 1, the consumer decides whether or not to drink a unit of alcohol. The immediate benefit of doing so is B. If he has purchased and stored alcohol at time 0, consumption involves no other immediate costs. If he has no supply at home, he can make a separate trip to buy some, but incurs an immediate cost, T, as a result of the inconvenience.

Time 2 represents the future, during which various other costs are incurred. Future costs result from drinking alcohol (which harms health) and spending money (which reduces future consumption). We use H and C to denote the costs associated with the former and the latter, respectively. In addition, if the consumer purchased alcohol at time 0 and did not drink it at time 1, he will receive some additional benefit from owning alcohol at time 2. Instead of deriving that value in the context of a full dynamic programming model, we simplify by assuming that the consumer must hold a unit of alcohol at the end of this period.[13] Consequently, if he does not car-

[13] Once could instead assume that the consumer can sell any stock he owns as of time 2; the results are the same.

ry stock into the period, he must purchase it at the cost C. He does so when shopping for other reasons, so that the inconvenience cost T is avoided.

In terms of objectives, we assume the consumer always maximizes the difference between immediate benefits and costs, plus a factor $\beta \in [0,1]$ times the difference between future benefits and costs. As a result, he is time-inconsistent.[14] However, β matters only at time 1: at time 0, he makes a decision but faces no immediate consequences, while at time 2, he makes no decision. We assume that the value of β applicable at time 1 is realized at the outset of that period, and that its distribution is governed by F, a CDF with support $\left[\underline{\beta}, \overline{\beta}\right]$. We treat β as random on the grounds that the severity of self-control problems depends on environmental cues that the consumer may or may not encounter, as in Bernheim and Rangel [2004]. We will assume for the moment that the individual is potentially time-inconsistent with respect to alcohol,[15] which requires $B - C - H < 0$, so that as of time 0 he wishes to avoid drinking at time 1, and $B - \underline{\beta}(C + H) > 0$, so that as of time 1 he may wish to drink at time 1.

Period 1 is the point in time at which sales restrictions on alcohol may apply – in other words, it represents "Sunday." At the outset, we will assume that no such restrictions are in place, and examine the consumer's decisions.

If the consumer holds a unit of alcohol at time 1, he will drink it as long as $B \geq \beta(C + H)$. Consequently, assuming he is sophisticated, his expected utility conditional upon purchasing a unit at time 0, from the perspective of time 0, is:

$$U_P = F\left(\frac{B}{C+H}\right)(B - C - H) - C.$$

In contrast, if he does not hold a unit at time 1, he will purchase one and drink it as long as $B - T \geq \beta(C + H)$. Because he ends up purchasing alcohol at time 2 regardless of whether he consumes at time 1, his expected utility conditional upon not purchasing a unit at time 0, from the perspective of time 0, is:

$$U_{NP} = F\left(\frac{B-T}{C+H}\right)(B - C - H - T) - C.$$

[14] In particular, he is a quasihyperbolic discounter.

[15] We classify the consumer as potentially time-inconsistent with respect to alcohol if his use of alcohol at time 1 could depend on the timing of his decision (i.e., whether he makes the choice at time 0 or time 1). Conversely, we classify him as time-consistent with respect to alcohol if his use of alcohol at time 1 definitely does not depend on the timing of his decision. Note that $\beta = 1$ is a sufficient condition for time-consistency with respect to alcohol, but it is not a necessary condition.

A sophisticated consumer controls alcohol consumption by keeping no easily accessible supplies iff $U_P \leq U_{NP}$,[16] or equivalently

$$TF\left(\tfrac{B-T}{C+H}\right) \leq \left[F\left(\tfrac{B}{C+H}\right) - F\left(\tfrac{B-T}{C+H}\right)\right](C + H - B).$$

Notice that refraining from purchasing alcohol at time 0 reduces total expected purchases from $1 + F\left(\frac{B}{C+H}\right)$ to $1 + F\left(\frac{B-T}{C+H}\right)$.

Depending on the values of the model's parameters, a sophisticated consumer may or may not choose to maintain a readily available supply. For instance, if $F\left(\frac{B-T}{C+H}\right) = 0$ (but $F\left(\frac{B}{C+H}\right) > 0$), so that the absence of readily available supply definitely deters drinking, we necessarily have $U_P < U_{NP}$, which means the consumer buys nothing at time 0. On the other hand, if $F\left(\frac{B-T}{C+H}\right) = 1$ (which implies $F\left(\frac{B}{C+H}\right) = 1$), the consumer drinks regardless of whether he maintains a supply at home, so $U_P > U_{NP}$; in other words, it is optimal to avoid the costs of inconvenience by stocking up in advance.

Now imagine that a law prevents sales of alcohol at time 1 ("Sunday"). Assuming the consumer stocks up at time 0, his outcome is unchanged: we have $U_P^B = U_P$ (where the superscript B denotes an outcome with a ban). However, if he fails to stock up, he can consume nothing at time 1, and hence his payoff is simply $U_{NP}^B = -C$. Because $B - C - H < 0$, we have $U_P^B < U_{NP}^B$, so he is unambiguously better off purchasing nothing at time 0. Consequently, his expected purchases fall to unity, irrespective of the parameters.

According to preceding analysis, a Sunday ban on liquor sales reduces off-premises purchases for two distinct reasons. First, it increases the effectiveness of the availability strategy conditional upon its use (the "efficacy channel").[17] Second, as a consequence of this increased effectiveness, a sales ban also enlarges the set of individuals who attempt to employ the availability strategy (the "usage channel").[18]

There are, of course, special conditions under which a Sunday ban has no effect on aggregate liquor purchases despite consumers' use of the availability strategy. First, without a ban, the availability strategy must be perfectly effective for all those who use it.[19] Second, again

---

[16] For the "iff" statement, we have resolved indifference in favor of no purchase.

[17] To understand why this is the case, notice that $1 + F\left(\frac{B-T}{C+H}\right) > 1$.

[18] To understand why this is the case, notice that $U_{NP}^B > U_{NP}$ and $U_P^B = U_P$, so $U_{NP} > U_P$ implies $U_{NP}^B > U_P^B$. Thus, the set of F, B, T, C, and H for which $U_{NP}^B > U_P^B$ is larger than the set for which $U_{NP} > U_P$.

[19] Perfect efficacy in the absence of a ban requires $F\left(\frac{B-T}{C+H}\right) = 0$, which implies that the individual would not make a special trip to the store to purchase liquor under any circumstances.

without a ban, there must be no "marginal" consumers for whom the net benefits of the availability strategy are not quite sufficient to justify its use. The first condition disables the efficacy channel, while the second disables the usage channel. We regard these conditions as highly unrealistic in light of other considerations. First, alcohol consumption is notoriously difficult to regulate, and the clinical literature implies that simple self-control strategies are far from perfect (see, e.g., Collins, 1993). Second, it is difficult to imagine a plausible distribution of underlying characteristics for which the alcohol-consuming population would divide between those who use and do not use the availability strategy while leaving no one on the margin.

So far, we have assumed that consumers are sophisticated. Similar conclusions follow if they are naïve. Because $B - C - H < 0$, the consumer does not anticipate his desire to consume alcohol at time 1; consequently he is indifferent between stocking up at time 0 and not stocking up (given that he must have a stock of one unit as of time 2). As long as some naïve consumers resolve their indifference in favor of not stocking up at time 0, they will inadvertently employ the availability strategy, which will be more effective with a Sunday ban (i.e., their total purchases will fall from $1 + F\left(\frac{B-T}{C+H}\right)$ to 1).[20] Thus, once again, the ban will reduce alcohol purchases.

Critically, our conclusion hinges on the assumption that the individual is potentially time-inconsistent with respect to alcohol, so that he uses availability as a self-disciplining device. For if $B - \underline{\beta}(C + H) < 0$, the consumer would purchase no alcohol under any circumstances, and if $B - \underline{\beta}(C + H) > 0$, he would always stock up on alcohol at time 0 to avoid the consequences of running out. Thus, the degree to which a ban on Sunday off-premise sales reduces overall alcohol consumption provides a gauge of the extent to which consumers are time-inconsistent with respect to alcohol, and are using availability (either intentionally or unintentionally) to exercise self-control.

Stepping outside of our simple model, a Sunday ban could reduce alcohol purchases irrespective of whether consumers are time consistent for two reasons: first, inventory-holding costs might discourage advance purchases; second, forgetful consumers might inadvertently neglect to stock up. Thus, strictly speaking, a finding that Sunday sales hours significantly affect liquor consumption would not rule out time consistency. However, we question whether either consideration could plausibly account for a large effect: the costs of holding inventories appear to be

[20] In a more elaborate model, naïve consumers might have reason to strictly prefer stocking up either at time 0 or at time 2. As long as some would choose not to stock up at time 0, the same conclusion would follow.

minor, and consumers who regularly drink liquor (and who therefore account for the bulk of sales) are likely to learn from experience and develop habits that help them avoid inadvertent depletion of supplies. More importantly, because these considerations would amplify rather than offset the effects of the availability strategy, a finding that liquor consumption is *unaffected* by Sunday sales hours would remain inconsistent with the hypothesis that significant numbers of consumers employ that strategy.

We have assumed above that a ban on "Sunday" off-premise sales makes alcohol unavailable, or equivalently that it increases the value of T by a large (prohibitive) amount. In fact, consumers may be able to obtain alcohol on Sundays at a higher but non-prohibitive cost by going to a bar or restaurant, in which case a ban effectively increases T by some smaller amount.[21] Strictly speaking, the effect of a small increase in T on total alcohol consumption is theoretically ambiguous: conditional on refraining from purchasing alcohol at time 0, consumption declines (because the effective price rises), but more people could end up purchasing alcohol at time 0 (e.g., if they know they would otherwise likely incur the higher costs of going to a bar). The latter possibility strikes us as rather perverse: it requires that, on the margin, an increase in inconvenience costs starting from T reduces the attractiveness of the availability strategy, even though that strategy is more attractive when those costs are T rather than zero,[22] and more attractive when they are B (i.e., prohibitive) rather than T. Still, in light of this theoretical possibility, we investigate whether the effect of a change in off-premise sales hours differs according to the length of on-premise sales hours as part of our empirical analysis.

Now consider the effects of a change in *on-premise* sales hours. As a first step, suppose the timing of the individual's trips to bars and restaurants is fixed. An important difference between off-premise and on-premise purchases is that alcohol is storable in the former context but not in the latter. Assuming that alcohol is complementary to restaurant meals and/or social interaction (so that consumption at home is an imperfect substitute), a partial or complete ban on Sunday sales will unambiguously reduce purchases. Next consider how changes in the timing of

[21] As is clear from Table 2, off-premises Sunday sales hours are generally shorter than on-premises hours. In principle, a consumer might also obtain alcohol on Sunday from friends. But anyone attempting to control alcohol consumption through the availability strategy would also have an incentive to encourage their friends not to share alcohol with them on Sundays. For those unable to control access through friends, the availability strategy is likely ineffective to begin with.

[22] When inconvenience costs are zero, the consumer's payoff is the same regardless of whether or not he purchases alcohol at time 0. Therefore, he will refrain from purchasing at time 0 when the inconvenience costs are T only if the availability strategy delivers a higher payoff with inconvenience costs of T than with inconvenience costs of zero.

trips affect this conclusion. A consumer who is time-consistent with respect to alcohol may shift his trips away from Sundays to avoid the ban. This response will mute the reduction in consumption to some degree, but not entirely, unless he is completely indifferent as to timing. A sophisticated time-inconsistent consumer with self-control problems may shift his trips to Sundays to take advantage of the ban (i.e., exposing himself to cues that could trigger alcohol consumption only when it is not available). That will magnify the reduction in consumption. Finally, a naïve time-inconsistent consumer with self-control problems will not adjust the timing of his trips, because he expects to refrain from consuming. Thus, regardless of the composition of the population, an abbreviation of on-premise Sunday hours should reduce alcohol purchases.

One limitation of our simple model is that it abstracts from considerations that might cause consumers to respond asymmetrically depending upon whether Sunday sales hours rise or fall. In states where Sunday sales bans have been in place for a long time, consumers may tailor their self-control strategies to exploit it, e.g., by making sure not to stock up on liquor before a weekend, or by eating out on Sundays. In contrast, in states where Sunday sales have always been permitted, consumers presumably develop other self-control techniques. Asymmetric responses to changes in Sunday hours are likely because the prevalent self-control strategies probably depend on the historical regime and evolve slowly in response to a change in that regime. Thus, for example, when on-premise Sunday sales are liberalized, alcohol consumption may rise by a larger amount in the short term than in the long term because consumers with self-control problems are in the habit of eating out on Sundays. When evaluating our findings, it is therefore important to bear in mind that states have, in general, progressively liberalized their regulations of Sunday liquor sales.

4. Data

Characterizing the evolution of each state's alcohol sales policies from 1970 through 2007 involved laborious archival research. We began by identifying the most recent statutes for each state. Using the amendment dates listed in the notes of the current statutes, we then searched for the pre-amended statutes, from which we determined the nature of the changes to the alcohol control laws. We collected information on the number of allowable hours for both on- and off-premises sales as well as the legal drinking age, in all cases noting whether localities were given discretion to set either more or less restrictive rules. We supplemented this infor-

mation with data compiled by the Distilled Spirits Council of the United States and published in its annual handbooks, which were available for a portion of the pertinent time period.

Stehr (2007) describes the patchwork of state alcohol regulations as Byzantine, but even that term understates their variety and complexity. For instance, some states prohibit the sale of alcohol unless food is also served; others have complicated rules that depend on a county's population. In many instances we found it challenging to characterize these laws along simple measurable dimensions. As a general rule, we measured allowable hours based on the least stringent statewide default rule, even if local jurisdictions were granted and frequently exercised discretion to deviate from the default. In cases where laws provide for contiguous hours of operation after midnight, we associate those hours with the previous day; for example, if a state allows sales from 10am on Saturday to 2am on Sunday but not otherwise on Sunday, we record 16 hours for Saturday and zero hours for Sunday. Several states provide no default and leave the regulation of hours (either on-premises, off-premises, or both) entirely up to local governments; three states did so for part of the period we study. We drop those state-year observations from the sample.[23]

Between 1970 and 2007, fifteen states repealed blue laws banning off-premises liquor sales on Sunday and fourteen states did so for on-premises sales. Including these repeals, twenty-one states increased allowable off-premise Sunday sales hours a total of thirty-four times, while thirty states increased allowable on-premise Sunday sales hours a total of forty-six times. All such changes are listed in Table 1.

Annual state-level data on liquor sales in gallons of pure ethanol equivalent come from the National Institute on Alcohol Abuse and Alcoholism. Using Census Bureau population data, we convert these figures into gallons of pure ethanol per person over the age of 18. Since these data describe sales rather than consumption, our results necessarily encompass effects on cross-border traffic (see, for example, Lovenheim and Slemrod, 2010, on evasion of the legal drinking age).[24] Another limitation of these data is that they are highly aggregated, not only across individuals, but also across off-premise and on-premise sales. While aggregate data suffice for the purpose of testing the hypotheses outlined in Section 3, they do not permit us to explore population heterogeneity with respect to the demand for and use of commitment devices.

[23] Including these observations with the default set to 24 hours (since, technically, a local govermnent could choose to set those hours) does not affect our results in a meaningful way. Full results provided on request.
[24] Note, however, that we use the terms "sales" and "consumption" interchangeably throughout the paper.

To separate cross-border effects from the impact on consumption among a state's residents, we control for differences between the regulations of neighboring states using two sets of variables. The first set is designed to control for the possibility that residents of the border counties of neighboring states travel into a state that has laxer laws than their own. For each state, we compute the number of allowable Sunday sales hours in excess of each of its neighbors (zero if it has shorter hours). We then multiply these excess hours by the number of people living in the border counties of the neighboring state, and sum over all neighbors to obtain the total number of potential person-hours driving inflows of customers (outflows of liquor). Finally, we divide by the original state's population to scale the variable in the same way as our dependent variable. We call the resulting variables OnInflows and OffInflows for on-premises and off-premises hours, respectively. The second set of variables is designed to control for the possibility that the state's residents cross borders to purchase alcohol in other states. For each state, we compute excess Sunday sales hours for each of its neighbors (i.e., the difference between the neighbor's hours and the state's hours, truncated below at zero). We then multiply these excess hours by the number of people living in the border counties of the subject state, and sum over all neighboring states to obtain the total number of potential person-hours driving outflows of customers (inflows of liquor). Again we divide by the subject state's population so that variables reflect comparable scaling. We call these variables OnOutflows and OffOutflows for on-premise and off-premise hours, respectively.[25]

Table 2 shows summary statistics. The mean consumption in our sample is nearly one gallon of ethanol equivalent per adult per year, though there is substantial heterogeneity both within and across states. The unconditional means for on- and off-premises Sunday sales hours are 10.0 and 4.9, respectively; conditional on positive hours, the means are 15.8 hours and 14.0 hours. Per-capita consumption is 5.5 percent higher for observations pertaining to states and years for which Sunday on-premise sales were allowed, compared with those for which such sales were not allowed, and 16 percent higher for observations pertaining to states and years for which Sunday off-premise sales were allowed, compared with those for which such sales were not allowed. However, these simple comparisons do not account for unobserved cross-state vari-

[25] These measures are similar to variables used by Stehr (2007), who uses an indicator for a Sunday sales ban instead of the difference in hours of sale.

ation in attitudes toward alcohol that likely affect both Sunday sales restrictions and consumption; nor do they account for other alcohol regulations or broader trends.

5. Results

The unit of observation in our analysis is a state-year. For all the specifications reported here, we regress the log of annual per capita ethanol-equivalent liquor sales on our measures of allowable Sunday on-premises and off-premises sales hours, state fixed effects (to capture differences in attitudes toward alcohol across states), year fixed effects (to capture general trends in alcohol consumption, macroeconomic activity, and other time-varying factors), and other controls. Thus, our central identifying assumption is that there are no systematic differences in unobserved factors affecting trends in alcohol consumption between states that do and do not change their policies regarding Sunday liquor sales. We employ standard diagnostics to evaluate the validity of that identifying assumption below. In all cases, the additional controls include variables capturing liquor and cigarette taxes,[26] the legal drinking age, provisions allowing local governments to expand or restrict Sunday sales hours relative to the statewide default,[27] the state's unemployment rate, and the state's total population. Standard errors are clustered at the state level. For brevity, we report coefficients only for key variables that capture aspects of policies targeting alcohol consumption; full results are available on request.

To interpret the magnitudes of the estimated effects, it is useful to know the fractions of total liquor consumption attributable to on-premises and off-premises Sunday drinking. To our knowledge, direct estimates of those fractions are not available. On the basis of indirect evidence, it appears that on-premises Sunday drinking accounts for between 5 and 10 percent of total liquor consumption, while off-premises Sunday drinking accounts for between 10 and 15 percent.[28] Therefore, if an on-premise ban suppressed all on-premise Sunday drinking without caus-

[26] The data on taxes were compiled and generously provided by Mark Stehr (see Stehr, 2007).
[27] Of the fifty-six legislative events that relaxed restrictions on Sunday sales hours (either on- or off-premises), thirteen were accompanied by changes in local governments' discretion to set either more or less restrictive rules.
[28] As a crude estimate of the fraction of liquor consumed on Sundays, one-seventh (14%) is conservative because people tend to drink more alcohol on weekends than on weekdays (Zegler, 2013). On the basis of indirect evidence, it appears likely that the actual figure is in the neighborhood of 20%. (According to NHTSA, 2003, 21% of fatal crashes involving a driver or pedestrian with BAC > 0.01% occur on Sundays; likewise, Rand et al., 2010, report that 20.5% of violent crimes involving alcohol occur on Sundays). In 2000, household spending on liquor was divided roughly evenly (53/47) between on-premise and off-premises purchases (Paulin, 2003; see also Bloom, 2012, and Zegler, 2012). Thus, if the price of liquor were the same on premises and off premises, each would account for roughly 10% of total liquor consumption. In fact, liquor is more expensive on premises than off premises. If we as-

ing substitution to other days, we would expect total alcohol sales to fall by 5 to 10 percent; similarly, if an off-premise ban suppressed all off-premise Sunday drinking, we would expect total alcohol sales to fall by 10 to 15 percent.

5.1 Main Results

Column (1) of Table 3 reports estimates for our main specification. Widening the allowable on-premise Sunday sales window by one hour is associated with a statistically significant 0.94 percent (s.e. = 0.29 percent) increase in sales. The point estimate implies that allowing twelve hours of Sunday on-premises sales increases total liquor consumption by roughly 11 percent. That estimate is somewhat larger than expected in light of the fact that, when allowed, Sunday on-premises sales likely account for less than 10 percent of liquor consumption. We return to this point below.

In contrast, expanding the allowable off-premise Sunday sales window by one hour is associated with a small and statistically insignificant 0.08 percent (s.e. = 0.24 percent) increase in sales. The point estimate implies that allowing twelve hours of Sunday off-premises sales increases total liquor consumption by roughly one percent. To put this figure in perspective, recall that if an off-premises ban suppressed all off-premise Sunday drinking, we would expect total alcohol sales to fall by 10 to 15 percent. Notably, despite the fact that more alcohol is consumed off-premises than on-premises, the estimated off-premises effect is much smaller than the on-premises effect, and the difference between them is statistically significant at p = 0.07. The hypothesis that consumers widely use the availability strategy is difficult to reconcile with this apparent absence of a meaningful relationship between liquor consumption and allowable off-premises sales hours, especially because other considerations such as forgetfulness and inventory-carrying costs would, if anything, tend to amplify the estimated effect (as explained in Section 3).

Other effects in column (1) are also of interest.[29] As expected (and consistent with the findings in Stehr, 2007), higher taxes on both liquor and cigarettes (which are complementary to

---

sume (consistent with some anecdotal evidence and "soft" numbers taken from the popular press) that restaurants and bars purchase liquor in bulk at a wholesale discount of 25% and sell it with a 300% markup over cost, then equal spending implies that on-premises liquor sales account for about one-fourth of Sunday consumption, or 5% of total consumption, while off-premises liquor sales account for about three-quarters of Sunday consumption, or 15% of total consumption.

[29] In addition to the effects discussed in this paragraph, the neighboring state controls are jointly significant (p = 0.006), with OffInflows and OnOutflows individually statistically significant, but some of the signs are counterintuitive.

liquor) reduce liquor consumption, and the effects are statistically significant. We are unable to detect a systematic relationship between liquor consumption and the legal drinking age, but the estimated effects are imprecise. A local option to expand allowable hours relative to the state default increases sales by 1.2 percent (s.e. = 2.3 percent), and a local option to restrict hours reduces sales by 4.6 percent (s.e. = 2.3 percent); only the latter effect is statistically significant.

The size of our data sample is somewhat constrained by the inclusion of controls for liquor and cigarette taxes, which we do not have for years before 1981. We use these controls to address a potential confound: if legislatures relax Sunday sales restrictions with the aim of increasing tax revenues, they may well raise tax rates simultaneously. Such occurrences would tend to offset any increase in alcohol sales resulting from longer sales hours. That said, dropping these variables allows us to add eleven years of data, which offers other potential advantages. Results appear in column (2) of Table 3. Our findings do not change appreciably, except that the estimated effect of a local option to expand allowable Sunday sales hours is now statistically significant.

In Section 3, we observed that the effects of a change in off-premise sales hours on the efficacy of the availability strategy, and hence on alcohol purchases, may differ according to the length of on-premise sales hours. In particular, the implication that those using the availability strategy should increase off-premise purchases in response to longer off-premise sales hours follows most cleanly when on-premise hours do not exceed off-premise hours. Thus, at least as a matter of principle, our failure to detect a significant effect of off-premise hours could be attributable to the fact that on-premise hours tend to be longer than off-premise hours.[30] To test this possibility, we added an interaction term between on-premise and off-premise hours to the specification. If longer on-premise hours weaken the effect of off-premise hours, the coefficient of the interaction term should be negative.[31] In fact, as shown in Column (3) of Table 3, that coefficient is small, *positive*, and statistically insignificant; moreover, the coefficient of off-premise hours becomes slightly negative. Consequently, we can rule out the possibility that the effect of off-premises hours has been muted by substitution to on-premises consumption.

In addition to regulating Sunday sales hours, some states also limit liquor sales on Saturdays and weekdays. With respect to such regulations, it is important to bear the following facts in

[30] Off-premise hours exceeded on-premise hours only in South Dakota.
[31] In making this statement, we invoke the reasonable assumption that the degree of substitution from off-premise to on-premise consumption is limited by the number of on-premise hours.

mind. First, changes in limits on non-Sunday sales hours have been much less frequent and generally much smaller than changes in limits on Sunday sales hours. For example, there were only 15 changes in maximum on-premises weekday sales hours in our sample, and those were confined to 10 states. The change exceeded one hour only seven times, and exceeded three hours only three times (those changes being 4, 9, and 11.5 hours). In contrast, there were 46 changes in maximum on-premises Sunday sales hours in our sample, and those were distributed over 30 states. The change was at least 10 hours thirteen times, and at least 4 hours twenty times. Second, though allowable hours on weekdays, Fridays, and Saturdays were all highly correlated with each other, they were much less correlated with allowable Sunday hours. For example, for allowable on-premises sales hours, weekday and Friday restrictions were highly correlated (ρ = 0.998), as were weekday and Saturday restrictions (ρ = 0.993), but the correlation between weekday and Sunday restrictions was comparatively modest (ρ = 0.271).

In light of these facts, we anticipated that the effects of allowable non-Sunday hours would be difficult to measure, and that the omission of controls for these restrictions would have little impact on the estimated effects of Sunday sales hours. These expectations are confirmed in column (4) of Table 3, which adds controls for allowable non-Sunday hours to our basic specification. The coefficients of these variables are negative but imprecisely estimated and hence not statistically significant. Notably, the coefficient of allowable on-premises Sunday hours is virtually unaffected, while that of allowable off-premises hours remains tiny but becomes slightly negative.

5.2 Tests of the Identifying Assumptions

As mentioned above, our central identifying assumption is that there are no systematic differences in unobserved factors affecting trends in alcohol consumption between states that do and do not change their policies regarding Sunday liquor sales. Thus, a threat to identification arises from the possibility that changes in laws affecting allowable Sunday sales hours were caused by state-specific changes in attitudes toward alcohol that also influenced consumption. The main concern is that legislators may have responded to increasing consumer demand for liquor by relaxing restrictions. In that case, the key coefficients in our specifications may be spurious. Notice, however, that this consideration would tend to bias the coefficients of interest *upward*. Consequently, it cannot account for the *absence* of a significant relationship between liquor consumption and off-premises sales, which is our central finding. Moreover, based on our

reading of the historical record, it appears more likely that liberalizations of sales hours have been driven primarily by orthogonal concerns about tax revenues. Still, the issue merits further investigation.

We address this issue in several ways. First, we add the one-year leads of allowable on-premise and off-premise Sunday sales hours to specification (1) from Table 3; see Column (1) of Table 4. If the concerns described above are indeed serious, the coefficients of the leads should be relatively large, indicating elevated consumption prior to the change in the law. Yet those coefficients are quite small and statistically insignificant. The addition of the leads reduces the coefficient for allowable on-premises sales hours to a more plausible magnitude, while the coefficient for allowable off-premise sales hours falls slightly to 0.0002 percent (s.e. = 0.26 percent). Including two leads does not change these results appreciably, as seen in Column (2) of Table 4.

Second, we also estimated specifications including lags of the key policy variables, which could enter with significant coefficients either because of confounding trends, or because liquor sales respond gradually to changes in sales hours. As shown in Column (3) of Table 4, the coefficients of the lags are statistically insignificant, and the effects of interest are largely unchanged. While measurement of a lagged effect does not permit us to distinguish between confounding trends and gradualism, there is no indication that either is important.

Finally, we estimated specifications that included interactions between a linear time trend and two dummy variables, one indicating whether a state ever changed off-premise sales restrictions during the sample period, and the other indicating whether it ever changed on-premise restrictions. Results appear in Column (4) of Table 4. The coefficients of the trend variables are small and statistically insignificant, and the main effects of interest remain largely unchanged.

Based on these tests, we find strong support for the central identifying assumption underlying our analysis.

5.3 Additional Robustness Checks

In light of the fact that we find a sizable effect of cigarette taxation on liquor consumption, one possible concern is that we may have controlled inadequately for the full range of policies that affect smoking. The most directly pertinent policies are those pertaining to smoking in places where people tend to consume alcohol. Accordingly, we added a dichotomous variable indicating whether state law prohibits smoking in bars. Results appear in column (1) of Table 5.

The effects of bans are statistically insignificant (-0.004, s.e. = 0.026), and the inclusion of this variable does not affect our main results.

Another possible concern is that changes in regulations affecting the sale of liquor may have coincided with similar changes affecting wine and beer.[32] To the extent liquor, wine, and beer are substitutes, such occurrences could potentially confound our results. We examine this possibility in two ways. First, we investigate whether the inclusion of tax rates for wine and beer affect our conclusions; they do not. As seen in Column (2) of Table 5, neither of those tax rates has a statistically significant effect on the consumption of liquor, and the coefficients for allowable hours of liquor sales are largely unchanged. Second, to control broadly for policies affecting wine and beer, we add variables measuring the total consumption of both. Those variables enter with large, statistically significant coefficients. This result is not surprising given that unobserved preferences for different types of alcohol are likely positively correlated, and that environments favoring the consumption of one type of alcohol will also likely favor the consumption of others. Still, the addition of these variables has virtually no effect on the coefficients of interest.

Finally, the specification in column (4) of Table 5 is identical to the one in column (1) of Table 3, except that we use dichotomous variables indicating whether Sunday on-premise and off-premise sales are allowed, rather than the number of hours. We include this specification primarily because it is more comparable to those in existing papers (see Section 2). According to this specification, eliminating a ban on on-premises sales causes a large and robust increase in sales, while eliminating a ban on off-premises sales causes a smaller and statistically insignificant increase. Taken together, the coefficients are similar in magnitude to those in Stehr (2007).

6. Conclusions

Taken as a whole, our results indicate that consumers increase their liquor consumption in response to extended Sunday on-premise sales hours, but not in response to extended off-premise sales hours. While our analysis uncovers no indication that off-premise sales hours affect alcohol purchases, it does not prove that such an effect is absent. As always, it is impossible to reject the hypothesis that an effect is non-zero. Still, in all specifications we fail to reject the

[32] Separately, we also examined whether Sunday sales hours for wine and beer affected wine and beer consumption. For wine, we obtain the same qualitative pattern as for liquor; however, the on-premise coefficient is not statistically significant (even though it is large economically). For beer, our results are puzzling: the estimated effect of *off-premise* hours is positive, though not significant in the richer specifications, while the *on-premise* effect is roughly zero. As we noted in the introduction, the results for liquor speak most directly and reliably to the hypotheses of interest because self-control problems are more prevalent for liquor (Kerr *et al*., 2002, and Roizen *et al*. 1999), and because sales restrictions are generally simpler and easier to code for liquor than for beer and wine.

hypothesis that the effect is zero (and thus that a demand for precommitment via the availability strategy is absent). Most strikingly, we fail to detect an effect of off-premise sales hours even though we easily detect a strong effect for on-premise sales hours. That disparity is at odds with the hypothesis that the demand for precommitment via the availability strategy is prevalent.

Thus, to our considerable surprise, we find no indication that precommitment strategies affecting availability play meaningful roles in aggregate liquor consumption. Instead, the observed pattern coincides with predictions for time-consistent consumers who have rational expectations and low costs of carrying inventories. Though the aggregated nature of the available data on liquor consumption precludes us from exploring population heterogeneity with respect to the demand for and use of commitment devices, we can nevertheless infer that use of the availability strategy is likely not widespread. Our findings also have important implications concerning tax policy and public health. Measures that relax restrictions pertaining to on-premise liquor sales are likely to increase state tax revenue, but may also entail greater alcohol-related social costs. However, measures that relax restrictions pertaining to off-premise liquor sales are unlikely to have either effect.

References

Amador, Manuel, Iván Werning, and George-Marios Angeletos (2006). "Commitment vs. Flexibility." *Econometrica* 74(2): 365-396.

Anton, R. F. (2001). "Pharmacological approaches to the management of alcoholism." *Journal of Clinical Psychiatry*, Vol. 62 (supp 20): 11-17.

Ariely, Dan and Klaus Wertenbroch. (2002). "Procrastination, Deadlines, and Performance: Self-Control by Pre-Commitment." *Psychological Science* 13(3): 219-224.

Ashraf, Nava, Dean Karlan and Wesley Yin. (2006). "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines." *Quarterly Journal of Economics* 121(2): 635-672.

Becker, Gary and Kevin Murphy. (1988). "A Theory of Rational Addiction." *Journal of Political Economy*, Vol. 96, No. 4, pp. 675-700.

Banerjee, Abhijit, and Sendhil Mullainathan (2010). "The Shape of Temptation: Implications for the Economic Lives of the Poor." Mimeo, MIT.

Bernheim, B. Douglas and Antonio Rangel. (2004). "Addiction and Cue-Triggered Decision Processes." *The American Economic Review*, Vol. 94, No. 5, pp. 1558-1590

Bernheim, B. Douglas, Debraj Ray, and Sevin Yeltekin. (2013). "Poverty and Self-Control." NBER Working Paper No. 18742.

Bryan, Gharad, Dean Karlan and Scott Nelson. (2010). "Commitment Devices." *Annual Review of Economics*. Vol. 2: 671-698.

Carpenter, Christopher and Daniel Eisenberg. (2009). "The Effects of Sunday Sales Restrictions on Overall and Day-Specific Alcohol Consumption: Evidence from Canada." *Journal of Studies on Alcohol and Drugs*. Vol. 70, No. 1, pp 126-133.

Beshears, John, James Choi, David Laibson, and Brigitte Madrian. (2011). "Self Control and Liquidity: How to Design a Commitment Contract." Working Paper.

Bloom, Beth. *White Spirits and RTDs – US – November 2012.* Mintel Group Ltd.

Choi, Laibson, Madrian. (2011). "100-dollar Bills on the Sidewalk: Violations of no-arbitrage in 401(k) Accounts." *Review of Economics and Statistics*. Forthcoming"

Collins, R. Lorraine (1993), "Drinking Restraint and Risk for Alcohol Abuse," *Experimental and Clinical Psychopharmacology* 1(1-4), 44-54.

DellaVigna, Stefano. (2009). "Psychology and Economics: Evidence from the field." *Journal of Economic Literature* 47(2): 315-372.

Distilled Spirits Council of the United States. *Summary of State Laws and Regulations Relating to Distilled Spirits.* Washington, D.C. Various years.

Diamond, Peter, and Botond Koszegi (2003). "Quasi-Hyperbolic Discounting and Retirement." *Journal of Public Economics* 87(9-10): 1839-1872.

Dupas, Pascaline and Jonathan Robinson. (2011). "Why Don't the Poor Save More? Evidence from Health Savings Experiments." NBER Working Paper No. 17255. July 2011.

Fang, Hanming, and Daniel Silverman (2009). "Time-Inconsistency and Welfare Program Participation: Evidence from the NLSY." *International Economic Review*, 50(4): 1043-1077.

Gine, Xavier, Dean Karlan, and Jonathan Zinman. (2010). "Put Your Money Where Your Butt Is: A Commitment Contract for Smoking Cessation." *American Economic Journal: Applied Economics* 2(4): 213-25.

Gruber, Jonathan and Botond Koszegi. (2001). "Is Addiction "Rational"? Theory and Evidence." *The Quarterly Journal of Economics* 116(4): 1261-1303.

Gul, Faruk and Wolfgang Pesendorfer. (2001). "Temptation and Self-Control." *Econometrica*, Vol. 69, No. 6, pp. 1403-1435.

Harding, Matthew, Ephraim Leibtag, and Michael Lovenheim. (2012). "The Heterogeneous Geographic and Socioeconomic Incidence of Cigarette Taxes: Evidence from Neilsen Homescan Data." *American Economic Journal: Economic Policy*, 4(4): 169-198.

Heaton, Paul. (2012). "Sunday Liquor Laws and Crime." *Journal of Public Economics*. Vol. 96, No. 1-2, pp. 42-52.

Hinnosaar, Marit. (2012). "Time Inconsistency and Alcohol Preferences." Working Paper.

Houser, Daniel, Daniel Schunk, Joachim Winter, and Erte Xiao. (2010). "Temptation and Commitment in the Laboratory," Institute for Empirical Research in Economics, University of Zurich, Working Paper No. 488.

Hughes, J. C., and C. C. H. Cook. (1997). "The efficacy of disulfiram: a review of outcome studies." *Addiction*, Vol. 92, pp. 381-395.

Kerr, William, Kay Middleton Fillmore, and Paul Marvy. "Beverage-specific alcohol consumption and cirrhosis mortality in a group of English-speaking beer-drinking countries." *Addiction*, 95(3): 339-346.

Kaur, Supreet, Michael Kremer, and Sendhil Mullainathan, (2010). "Self-Control at Work: Evidence from a Field Experiment." Working Paper.

Krusell, Per, Burhanettin Kuruscu and Anthony Smith (2000). "Tax Policy With Quasi-Geometric Discounting." *International Economic Journal* 14(3): 1-40.

Laibson, David (1997). "Golden Eggs and Hyperbolic Discounting." *Quarterly Journal of Economics*, 112(2): 443-78.

Laibson, David (1998). "Life-Cycle Consumption and Hyperbolic Discount Functions." *European Economic Review* 42: 861-871.

Laibson, David, Andrea Repetto, and Jeremy Tobacman (1998). "Self-Control and Saving for Retirement." *Brookings Papers on Economic Activity*, 1: 91-172.

Ligon, Jan and Bruce Thyer. (1993). "The Effects of a Sunday Liquor Sales Ban on DUI Arrests." *Journal of Alcohol and Drug Education*, 38: 33-40.

Lovenheim, Michael, and Joel Slemrod. (2010). "The fatal toll of driving to drink: The effect of minimum legal drinking age evasion on traffic fatalities." *Journal of Health Economics*, 29(1): 62-77.

Lovenheim, Michael and Daniel Steefel. (2011). "Do Blue Laws Save Lives? The Effect of Sunday Alcohol Sales Bans on Fatal Vehicle Accidents." *Journal of Policy Analysis and Management*. Vol. 30, No. 4, pp. 798–820.

McMillan, Garnett P. and Lapham, Sandra. (2006). "Legalized Sunday Packaged Alcohol Sales and Alcohol-Related Traffic Crashes and Crash Fatalities in New Mexico." *American Journal of Public Health*, 96(11): 1944-1948.

National Highway Traffic Safety Administration (NHTSA), National Center for Statistics and Analysis (2003). *2002 Annual Assessment of Motor Vehicle Crashes Based on the Fatality Analysis Reporting System, the National Accident Sampling System and the General Estimates System.* Washington, DC: U.S. Department of Transportation.

O'Donoghue, Ted and Matthew Rabin. (1999). "Doing It Now or Later." *The American Economic Review*, Vol. 89, No. 1, pp. 103-124.

Olsson, O. and P.O.H. Wikstrom. (1982) "Effects of the experimental Saturday closing of liquor retail stores in Sweden." *Contemporary Drug Problems*, 11:325-353.

Ornstein, Stanley I. and Hanssens, Dominique M. (1985). "Alcohol Control Laws and the Consumption of Distilled Spirits and Beer." *The Journal of Consumer Research*, 12(2): 200-213.

Paulin, Geoffrey (2003), "Consumer Expenditures for Alcohol in 2000," *Consumer Expenditure Survey Anthology*, 39-59.

Rand, Michael R., William J. Sabol, Michael Sinclair, and Howard Snyder (2010), "Alcohol and Crime: Data from 2002 to 2008," mimeo, Bureau of Justice Statistics, U.S. Department of Justice.

Roizen, Ron, William Kerr, and Kaye Fillmore. (1999). "Cirrhosis mortality and per capita consumption of distilled spirits, United States, 1949-94: trend analysis." *BMJ*, 319(7211): 666-670.

Russell, Jenna. (2003). "Sunday Liquor Sale Ban to End." *Boston Globe*, November 23.

Severson, Kim. (2011). "States Putting Hopes in 'Bottoms Up' to Help the Bottom Line." *The New York Times*. September 28, 2011. Page A1.

Smart, R.G. (1977) "The relationship of availability of alcoholic beverages to per capita consumption and alcoholism rates." *Journal of Studies on Alcohol*, 38:891-896.

Smith, D. Ian. (1990). "Effect on Casualty Traffic Accidents of Changing Sunday Alcohol Sales Legislation in Victoria, Australia." *Journal of Drug Issues*, 20: 417-426.

Stehr, Mark. (2007). "The Effect of Sunday Sales Bans and Excise Taxes on Drinking and Cross-Border Shopping for Alcoholic Beverages." *National Tax Journal*. Vol. 60, No. 1, pp. 85-106.

Stehr, Mark F. (2010) "The Effect of Sunday Sales of Alcohol on Highway Crash Fatalities," *The B.E. Journal of Economic Analysis & Policy*: Vol. 10: Iss. 1 (Topics), Article 73.

Thaler, Richard, and Shlomo Benartzi (2004). "Save More Tomorrow: Using Behavioral Economics to Increase Employee Saving." *Journal of Political Economy*, 112: S164-S187.

Zegler, Jennifer (2012), *Dark Spirits – US – September 2012.* Mintel Group Ltd.

Zegler, Jennifer (2013), *Alcohol Consumption at Home – US – May 2013.* Mintel Group Ltd.

Table 1: Changes in Hours of Sale

State	Year	On- or Off-Premises	Original Hours	New Hours
Arizona	1986	Both	13	15
Arizona	2004	Both	15	16
Arkansas	1999	Both	0	10
Colorado	1997	On	12	19
Connecticut	1993	On	0	15
District of Columbia	2004	On	17	16
Florida	1972	Both	0	17
Indiana	2004	On	0	10
Iowa	1974	Both	0	10
Iowa	1984	Both	10	14
Iowa	1991	Both	14	18
Kansas	1986	On	0	17
Maine	1976	On	0	13
Maine	1991	Off	0	5
Maine	1993	Off	5	13
Maine	1995	Both	13	16
Massachusetts	2004	Off	0	12
Massachusetts	2004	On	0	13
Michigan	1976	Both	10	12
Michigan	2004	Both	12	14
Missouri	1986	On	0	12
Missouri	1993	Off	0	11
Missouri	1993	On	12	13
Missouri	1994	Off	11	13
Missouri	2001	Both	13	15
Missouri	2004	Both	15	19.5
Montana	1975	Both	13	18
Nebraska	1991	Both	0	13
New Hampshire	1994	Off	14.75	17.75
New Hampshire	1994	On	16	19
New Mexico	1995	Off	0	12
New York	1976	On	14	16
North Carolina	1971	On	10.75	13
North Carolina	1993	On	13	14
North Dakota	1993	Both	0	13
North Dakota	2004	Both	13	14
Ohio	2001	On	11	14
Oregon	2002	Off	0	15
Pennsylvania	1975	On	9	13
Pennsylvania	1984	On	13	15
Pennsylvania	2002	Off	0	5
Rhode Island	2004	Off	0	6
South Dakota	1989	Off	0	17
South Dakota	1991	On	11	13
South Dakota	2004	On	13	15
Texas	1993	On	12	14
Utah	1985	On	12	11
Utah	1993	On	11	12
Utah	2004	On	12	15
Vermont	1973	Off	9	14
Vermont	2001	Off	14	18
Vermont	2001	On	16	18
Virginia	2004	Off	0	11
Washington	1976	On	12	20
West Virginia	1983	On	0	11
West Virginia	1986	On	11	13
West Virginia	2004	On	13	14
Wisconsin	1988	On	17.5	20
Wyoming	1973	Both	7	10
Wyoming	1996	Both	10	20

Table 2: Variable Definitions and Summary Statistics

Variable	Description	Mean	Standard Deviation
Sales	Per-capita distilled spirits sale (gallons of ethanol equivalent)	0.972	0.472
LogSales	Log of ethanol-equivalent sales	-0.111	0.384
AllowSundayOnPremises	1 if the state allows on-premises Sunday sales	0.657	0.475
AllowSundayOffPremises	1 if the state allows off-premises Sunday sales	0.334	0.468
SundayOnHours	Number of on-premises Sunday sales hours	10.0	7.70
SundayOffHours	Number of off-premises Sunday sales hours	4.89	7.25
UnemploymentRate	State-level unemployment rate	5.88	2.01
PopOver18	State population over 18 years of age in millions	3.69	4.16
LegalAge18	1 if the state drinking age is 18 in that year.	0.083	0.272
LegalAge19	1 if the state drinking age is 19 in that year.	0.076	0.258
LegalAge20	1 if the state drinking age is 20 in that year.	0.031	0.162
LegalAge21	1 if the state drinking age is 21 in that year.	0.811	0.389
Expand	Local option to expand on- or off-premises Sunday hours from the default.	0.501	0.500
Restrict	Local option to restrict on- or off-premises Sunday hours from the default.	0.426	0.496
OnInflows	Number of on-premises sale hours in excess of neighbor states, weighted by neighbor state border county population over own state population	2.66	7.74
OffInflows	Number of off-premises sale hours in excess of neighbor states, weighted by neighbor state border county population over own state population	2.08	7.05
OnOutflows	Number of on-premises sale hours less than neighbor states, weighted by own state border county population over own state population	3.21	7.59
OffOutflows	Number of off-premises sale hours less than neighbor states, weighted by own state border county population over own state population	2.71	6.06

This table reports means and standard deviations for 1722 observations across 47 states that had a state default for both on- and off-premises hours of sale for at least one year.

Table 3: Main Results

Variable	(1)	(2)	(3)	(4)
SundayOnHours	0.0094*** (0.0029)	0.0116** (0.0035)	0.0093*** (0.00288)	0.0099*** (0.0032)
SundayOffHours	0.0008 (0.0024)	0.0012 (0.0025)	-0.0051 (0.0049)	-0.0011 (0.0026)
SundayOnHours x SundayOffHours	-	-	0.0004 (0.0003)	-
NonSundayAverageOnHours	-	-	-	-0.0033 (0.0058)
NonSundayAverageOffHours	-	-	-	-0.0107 (0.0071)
Liquor Tax (log of real dollars per gallon)	-0.0321** (0.0152)	-	-0.0508** (0.0229)	-0.0293* (0.0157)
Log of Cigarette Tax (log of real dollars per pack)	-0.0278** (0.0125)	-	-0.0281** (0.0122)	-0.0235* (0.0131)
LegalAge18	0.0700 (0.0743)	0.0482 (0.0397)	0.0754 (0.0716)	0.0473 (0.0818)
LegalAge19	-0.0346 (0.0204)	-0.0057 (0.0235)	-0.0290 (0.0209)	-0.0378* (0.0220)
LegalAge20	-0.0185 (0.0219)	-0.0381 (0.0226)	-0.0152 (0.0225)	-0.0324 (0.0253)
Expand	0.0120 (0.0203)	0.0483** (0.0218)	0.0083 (0.0200)	0.0096 (0.0194)
Restrict	-0.0458** (0.0231)	-0.0640** (0.0232)	-0.0393 (0.0242)	-0.0334 (0.0216)
N	1196	1722	1196	1050

Coefficients are reported for OLS specifications with the log of per-capita alcohol sales as the dependent variable. Each regression also includes state and year fixed effects, controls for restrictions on liquor sales hours in neighboring states, the state's unemployment rate, and the state's total population. Standard errors clustered at the state level are in parentheses; those significant at the 10% level are marked with * and those significant at the 5% level are marked with **.

Table 4: Tests of the Identifying Assumptions

Variable	(1)	(2)	(3)	(4)
SundayOnHours	0.0077*** (0.0025)	0.0071*** (0.0023)	0.0062** (0.0025)	0.0087*** (0.0029)
SundayOffHours	0.0002 (0.0026)	0.0008 (0.0025)	0.0000 (0.0025)	0.0009 (0.0030)
SundayOnHours – Lead	0.0014 (0.0017)	-0.0010 (0.0025)	-	-
SundayOffHours – Lead	0.0009 (0.0019)	-0.0010 (0.0021)	-	-
SundayOnHours – Two Leads	-	0.0029 (0.0031)	-	-
SundayOffHours – Two Leads	-	0.0019 (0.0027)	-	-
SundayOnHours – Lag	-	-	-0.0007 (0.0008)	-
SundayOffHours – Lag	-	-	-0.0001 (0.0007)	-
SundayOnHours – Two Lags	-	-	0.0023 (0.0017)	-
SundayOffHours – Two Lags	-	-	0.0003 (0.0016)	-
EverOnChange x Trend	-	-	-	0.0011 (0.0019)
EverOffChange x Trend	-	-	-	0.0011 (0.0021)
N	1149	1102	1104	1196

Coefficients are reported from an OLS model with the log of per-capita alcohol sales as the dependent variable. Each regression also includes a set of indicators for the state's legal drinking age, indicators for whether local jurisdictions can expand or restrict hours of sale, unemployment rate, population, neighboring state controls, the log of liquor and cigarette taxes, and state and year fixed effects. Standard errors clustered at the state level are in parentheses; those significant at the 10% level are marked with * and those significant at the 5% level are marked with **.

Table 5: Additional Robustness Checks

Variable	(1)	(2)	(3)	(4)
SundayOnHours	0.0095*** (0.0028)	0.0095*** (0.0028)	0.0089*** (0.0028)	-
SundayOffHours	0.0008 (0.0024)	0.0008 (0.0023)	0.0003 (0.0024)	-
AllowsSundayOn	-	-	-	0.0805** (0.0366)
AllowsSundayOff	-	-	-	0.0367 (0.0297)
Ban on Smoking in Bars	0.0038 (0.0259)	-	-	-
Wine Tax (log of real dollars per gallon)	-	0.0032 (0.0200)	-	-
Beer Tax (log of real dollars per gallon)	-	0.0595 (0.0738)	-	-
Log of Beer Consumption	-	-	0.324*** (0.0890)	-
Log of Wine Consumption	-	-	0.104** (0.0397)	-
N	1196	1196	1196	1196

Coefficients are reported from an OLS model with the log of per-capita alcohol sales as the dependent variable. Each regression also includes a set of indicators for the state's legal drinking age, indicators for whether local jurisdictions can expand or restrict hours of sale, unemployment rate, population, neighboring state controls, the log of liquor and cigarette taxes, and state and year fixed effects. Standard errors clustered at the state level are in parentheses; those significant at the 10% level are marked with * and those significant at the 5% level are marked with **.